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Centro de Lógica,
6th May, 1982.

Dear Michael,

Thanks for your letter.

I enclose the report on our joint paper that you requested – I hope it's satisfactory and says what needs to be said. I don't know whether these things are supposed to be confidential, but as far as I'm concerned, I'd be happy for you to read it if you so wish. Naturally, if there is anything in it that strikes you as unsatisfactory or not appropriate for the desired ends, you should let me know.

I received your paper with Heywood with special interest. You probably do not recall it, but sometime in my last two years at Chelsea – I don't remember when exactly – I showed you an attempted proof that the Gleason-Kochen and Specker result led to non-locality for joint systems, that I had arrived at. I first showed it to Jon, and although the proof was pretty informal, he accepted it, but then you pointed out that the proof was essentially incomplete. I put the thing aside, but I always felt that it was intuitively plausible that contextuality could be shown to lead to some sort of violation of locality for joint separated systems when dealing with locally maximal observables. So I was interested to read in your paper

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on Locality and Relativity a reference to the work of Stairs, although I didn't have access to his thesis. At any rate, you and Heywood are to be congratulated on what looks like a very pretty result. I have still to read the paper with care, but I'm sure you are right. I think you are absolutely right too to avoid Fine's "conspiracy" approach – much as I admire his papers on the measurement problem I think his more recent work is discoloured by a touch of "misplaced slyness".

My own work has been interrupted again by a sea of administrative duties in the department – the bureaucracy here is hellish and there is no equitable distribution of tasks among the members of the department. Fortunately, my duties end soon and I'll have time to get back to real work. My article on the Bohr-Einstein debate came out at the end of last year in our journal (the number in question should have been routinely sent to you as one of the Advisors). In the article I attempted to show in some detail that, contrary to the common view, the development of E's criticism of QM between 1927 and 1935 was essentially continuous and that no radical break occurred therein between the "defeat" at the Solvay Conferences and the EPR paper.

Thanks for your information about Sérgio. I didn't know he was living in Vienna. Your suggestion concerning Stephen French sounds like a good one. Unfortunately, the question of new positions here is again somewhat up in the air, as a number of staff members have suddenly decided to leave and the whole group, with its aims and staff requirements, is being subject to re-examination. I fear that the area concerned with Phil/Hist of physics may not come out well in the re-shuffling, as many people here feel (unwisely I think) that

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it is too specialised and that we should strengthen the group in general epistemology and methodology of science. My own position is not threatened, but my plans to fortify the physics group may come to grief. At any rate, I should remark that if there still is a position open next year in our group, it will not be until July/August (and not January). Nonetheless, I would be grateful to have Stephen's curriculum and would like to keep in touch with his progress and plans. You might suggest that he write to me directly.

As for my health – I'm quite OK and recovered from a minor ailment that developed some months ago. I have no idea how Sérgio got to hear about it, but no doubt the story grew in the telling. Thanks for your concern, anyway.

While on the subject, I hope you, Jenny and the boys are well. Say hello to Allan for me.

Best regards,
Harvey.

P.S. I very much hope that your application for the D.Sc. is as successful as it is well-deserved.
H.

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Head of Department
H R Post MA PhD

1.3.82

Dear Harvey,

Thanks for your letter.

My paper on Nonlocality, Causality and the EPR Paradox is to be published in R. Swinburne (ed.): 'Space Time and Causality' (D. Reidel) 1982.

With regard to your remarks about Eberhard's proof, I agree the analysis of counterfactuals is always a contentious issue in philosophy, but if we adopt the Lewis-Stalnaker approach then I agree that PLCD is all right in the case of determinism but is not all right in the indeterministic case. But other analyses of $\Phi \Box\!\!\rightarrow \psi$ can produce different results about this. Ultimately one is reduced to arguing about one's intuitions – mine tend

PTO

to agree with the analysis given in my paper, but intuitions are not proof!

I am glad you are now writing up the State substitutes respect state evolution approaches to the Measurement problem. I am sure you are right about that.

With regard to the job in History/Phil. Science in January 1983, I am not sure it would suit Sergio. His abilities are really in mathematics rather than physics. Also he still has not written his thesis. At the moment he is living in Vienna, and coming to see me occasionally. But until the thesis is written I cannot really say whether he is going to get a Ph.D. at all!

I think a better bet for you would be <u>Stephan French</u> who is completing his Ph.D. this summer in Geology and

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II

Individuality in Classical and Quantum Physics. Stephen has a degree in Physics (from Newcastle) and is very conscientious and a really able student. He has recently married an American girl, and would be happy to apply formally for the post at Imperial if you are interested in getting him. Let me know what you think.

I enclose a new proof of nonlocality that does not employ the Bell inequality. I have written the paper with Peter Heywood, who is now well on his way to a Ph.D and we have submitted it to Foundations of Physics. Any comments would

pgs

be appreciated.

May I ask you a favour.

In order to buttress my own position at Chelsea, Heinz is very keen for me to apply for the D.Sc. This is based on published work and in the case of joint publications I need written confirmation from my co-author of the our relative roles in the research. I wonder if you ~~you~~ could let me have a note about our joint paper, addressed "To whom it may concern - - -" (For this purpose, you can be as generous as you like!)

Sergio had told that you had not been well - I hope this was nothing serious, and you are now quite recovered.

Yours

Rabi

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Centro de Lógica,
UNICAMP, Campinas, S.P.
Brazil.

8th February, 1982.

Dear Michael,

Thanks for the recent papers. I hope your talk went well last year on the fine causality / EPR-Bell / tachyon paper. (If you ever feel up to it, I'd be grateful to have any comments on my comments on your treatment of the original Eberhard ~~to~~ inequality proof). This brings me to the principal point of this letter — I'd like to know if you have submitted the paper (or a version of it) for publication, and if so, where. I am finally writing up a ~~[illegible]~~ belated draft of my stuff on the insolubility proofs of the measurement problem and I'd like to cite your paper, if possible, in relation to your account of the various notions of non-locality in QM. My work on the insolubility proofs has now expanded to such an extent that I think I'll have to submit it as two papers. I'm thinking of the International Journal of Physics, which has published a considerable amount of stuff in the same area. I'll send you

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a copy when its ready, hopefully in about a month.

We have had here since last August a student of Shimony's, Maria Penha Dias (a Brazilian) giving the first courses in our programme on the history of physics. She is a physicist by training, gave a good course on the history of mechanics and astronomy from Aristotle to Newton and this semester will teach the history of statistical mechanics. (She worked with Shimony on Jaynes' interpretation of SM and its implications in inductive logic). However, the department has decided that the position she holds should go to someone with some grounding also in epistemology - which she lacks. So the position for historian-philosopher of science will be open, probably after January of 83. In this respect I would like to ask you some questions about Sérgio. He would be a possible candidate, and I have written to him asking for his C.V., but have received no reply as yet. I understand he intends to return to Brazil shortly.

Where has he arrived with his research, and what is the state of his thesis? I take it that ~~you~~ he has done most of his work with you. Could you give me some kind of reference - it will

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obviously be treated as confidential. Were he to be accepted for the position, he would probably start by giving courses in the history of physics. Do you think he would be a good investment, so to speak, for our programme?

We have invited Allan down for two months in the second semestre to give seminars on his recent work, and are waiting to see if we can get funding for his trip. It will be nice to see him again (I think he mentioned in one of his letters that you are going to Colorado later in the year?). Would you consider a similar visit here, say in 83 or 84? There would be a good chance of our being able to ~~fund~~ provide travel expenses and maintainence. Think about it. My colleague Vincent Bronomano from Mathematics is also interested in getting Shimony for a month in '83. Last year's visit by Erasmo Recami was fairly successful, and in fact he seems to be interested in a position at UNICAMP

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in the Maths Institute, although I believe his chances are slim.

Thanks in advance for considering all these queries. I hope you and the family are well – give them mine and my wife's best regards.

Harvey.

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Centro de Lógica,
7th August.

Dear Michael,

I am belatedly getting this letter off to you with some long promised comments on your very interesting paper.

Before I get into the paper, I want to thank you for sending me the reprints of our article. I've received about nine requests from a number of countries - including East Germany - and I've sent off the reprints to all these people.

Sérgio called me from Brasilia just before he left for England. Unfortunately, he wasn't able to get to São Paulo so I missed out on seeing him.

About the paper - I have enjoyed reading it, and it has given much food for thought. I've distributed copies to a few of the people here interested in the foundations of physics. As a matter of fact, Erasmo Recami is due here in about a week - & we've invited him for two months - and if all goes according to plan, we will be using your paper as the basis of discussion in some of the seminar - discussion groups we will have with him. I liked in particular your treatments in sections 1-6 of the question of the types of non-locality, and your use of the Stapp - Eberhard formalism in the treatment of the Bell inequality.

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Unfortunately, I haven't as yet been able to familiarise myself sufficiently with the background material related to sections 7-8 to make any detailed comments here. I have so many questions and doubts, not about the logic of your arguments, but about what is essentially going on in the Grünbaum-Reichenbach-Salmon approach that I prefer not comment until I've got a more coherent picture of the significance of the synchronisation procedures and their relationship with the I.P.[1] Nonetheless, I may say that your conclusions seem plausible — non-existence proofs in physics (especially those due to philosophers!) are, or should be, a priori highly suspect and it seems that you have done to the Grünbaum, Salmon, etc line against tachyons what Bell did to the von Neumann proof against hidden variables in QM. (I would like to know sometime whether you accept that the simplified version of SR (p38) is the correct interpretation of the theory).

~~Before I return to the first part of the paper on non-locality,~~

What follows are some remarks concerning the first part of the paper.

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1. One question: Is not the restriction (p31) to subluminal frame velocities somewhat arbitrary?

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i) Locality$_1$ and Locality$_3$.

In the EPR argument, the (sufficient) criterion of reality tells us that immediately after the measurement of $S_z(1)$ at t_1 (p 2), the correlated value of $S_z(2)$ is an element of reality. The question is whether the element of reality for system 2 existed prior to t_1.

For Einstein, clearly the locality assumption was concerned with the denial of an instantaneous action of this measurement at t_1 on system 2. Instantaneous action-at-a-distance was distasteful to Einstein because it ressuscitated the ghost of absolute simultaneity[1], so was was for him in conflict with SR. It is interesting to note that if this action involves an energy transfer (although it's not clear to me that in the EPR case it should) then not even the extended SR can be used to "explain" the effect since transcendent tachyons have zero energy. At any rate, the effect, if it exists, has to be instantaneous for Einstein, since if the correlated value of $S_z(2)$ is "actualised" at $t_1 + \Delta t$, the Einstein criterion of reality is invalidated during the interval Δt.

Still, there is the possibility (as in the case of Locality$_3$) that the physical choice and setting up of the apparatus at t_0 can somehow be communicated (subluminally or at vel c) to the source of the particles, supposedly affecting the process of emission at t_0', say. ~~in such a way that system 2 is emitted with the required chiral~~ ~~ity (common sol~~ But it is not difficult to see that there is no way that this mechanism can save the EPR locality$_1$ vs. completeness dilemma, although it may work in principle in the case of rescuing

$(t_0 < t_0' < t_1)$

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1. Schilpp vol I. p. 61. (Einstein number)

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Bell locality$_3$. (Hence Aspect).

This may all be obvious but it does bring out ~~the~~ an important difference between locality$_1$ and locality$_3$; you correctly say (p 6) that we must give up locality$_1$ even in the Einstein sense (if we ~~retain~~ maintain completeness), whereas violation in the Einstein sense of locality$_3$ (p 13) is still an open question.

* *

By the way – do you happen to have any information about Aspect's experiment? It seems to be taking an awfully long time. My friend Buonomano from the Maths department here who has published a 'non-ergodic' theory – an attempt to justify a particle interpretation of QM based on an ether with peculiar properties; rather crazy but what can you expect? – Buonomano has been in touch with Aspect, who seems somewhat reticent about details of the progress of the experiment.

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ii) The Stapp - Eberhard Approach. (Section 4).

I found your argument convincing against the Stapp - Eberhard (S-E) version of the non-locality (Loc_4) result. But I wonder whether your point based on the difficulties imposed by ~~the~~ indeterminism in the analysis of counterfactual definiteness, even if the strongest, is the only consideration against the S-E approach. You assume that the PLCD is innocuous in the case of determinism. It wouldn't surprise me if many people were to find the use of the PLCD somewhat questionable even in the case of determinism.

The term γ_n refers to the nth particle pair and the $a_n b_n$ etc. are defined as being outcomes of actual ~~or~~ ~~con~~ and counterfactual measurement outcomes on that pair. It is this mixture of the real and counterfactual which seems curious. Let me see if I can expand on this point a little.

In your example of the radium atom, the question of truth conditions for $\phi \Box\!\!\rightarrow \psi$ is a question concerning the existence of other worlds W_j (close to ours) in each of which $W_j(\phi) \rightarrow W_j(\psi)$. Thus the counterfactual in question is taken to mean something about any one of the ~~given~~ worlds W_j ($W_j \leq_i W_k$). This seems reasonable. Similarly, the 'counterfactuality' in your construction of the Bell inequality via the Eberhard ~~note~~ expression is 'innocuous' because the a_n, b_n etc. refer to possessed values for properties in ~~this~~ one (this) world. But in the case of ~~the~~ spin-correlation the question of the definiteness of γ_n is a question about four distinct worlds, even in the deterministic case. What significance does γ_n have for our world?

If we use your diagram on page 11 to illustrate the S-E approach, the numbers in the columns refer not now to possessed values in this world, but measurement outcomes in four worlds (one being ours). The numbers which correspond to our world will once again define a place selection, as for the case of possessed values. Thus in both cases, there are unknown

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numbers in the columns; although we may accept the meaningfulness (pace Bohr) of real but unknowable entities in our world, it seems disconcerting to be discussing unknowable entities in these other worlds distinct from ours and mixing them (in the definition of γ_n) with those known entities in ours, only to compare the result with with what QM predicts in our world (or in any of the <u>individual</u> possible close worlds).

I don't know whether I can articulate my misgivings against the S-E line in a clearer fashion. It seems to me we may consider the problem of assigning truth conditions to two kinds of counterfactual claims – the first innocuous: If I were in my office now (I'm not), I would find three pens on the table; the second less so (a favourite example of Popper's!): If my grandmother had four wheels she would be a bus. Isn't the S-E line a case of the second kind?

~~Note that your point based on indeterminacy also goes through even in the deterministic case, but when the hidden variables are unknown or uncontrollable in each possible world which obeys QM.~~

I may remark that <u>if</u> the very definition of γ_n in the S-E argument is, for such reason, considered ~~to~~ illegitimate in principle, then we could turn the argument around and assuming Locality, ~~and~~ <u>derive</u> the impossibility of simultaneous measurements

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an, an', etc. (See p. 308 of my thesis).

* *

Finally, I noticed the following (probably typing) errors in the text. In footnote 1 (p 40) you refer to Redhead [1980], not listed in the references. In footnote 36 (46) you cite Podlaha and Navrátil [1966] – in the references (p 54) the date is [1967].

My own plans for writing have been pushed back a little – but this semestre I'll have a lot more time for my own work. My article on the Einstein - Bohr debate comes out in the Cadernos next month, and I intend to send you an English version shortly. The measurement paper based on the first part of the thesis is almost at the typing stage.

Could I ask you to send me a reprint (if you have any) of your article in the BJPS examining Donald's falsiprobability rule in probability? The number with your paper is missing in São Paulo. Thanks.

I hope you and the family are well. Maria-Rita and I send our fond regards.

Harvey.

P.S. I hope the ~~article~~ paper is well - received at the Conference.

H

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34 Coniger Rd
London S.W.6

5th October

Dear Harvey,

Thank you for your letter.
I agree with your new footnotes referring to the papers of Roychoudhuri and Yoshikuni. With regard to my Footnote F I intended this to refer to a fully quantised system interacting with an unquantised system. My point is that in such a hybrid theory one could not simply equate q-numbers and c-numbers. One way of getting a c-number out of a q-number is to take the expectation value. (Rosenfeld makes a similar point in his Nuclear Physics paper with regard to unquantised gravitation). The detailed formalism of such a hybrid theory might involve some unusual physical effects (such as conservation laws on the average) but does not involve logical incoherence.
I think the point made in the footnote is quite an important one and should be left in. (I enclose a slightly amended form of the footnote).
I am glad to hear all is going well at the Centro. Do you now feel with hind-sight that you made the right decision in going to Campinas?

With best wishes
Yours sincerely.
Michael

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Centro de Lógica 15 July.

Dear Michael,

Many thanks for your letter and copy of the paper on the Sum Rule. It was good to get your news — you seem to be as busy as ever!

I've only had time to skim through your paper, although it does seem to me that you have cleared up some seriously loose ends in the physics in the Fine - Cartwright - Glymour debate. I hope in a few weeks to have more time to sit down and read the paper more thoroughly.

While on QM, I have had a number of discussions with a mathematician here called Vincent Buonomano whose chief interest now is the Bell Result. He has developed what he calls a 'non-ergodic' interp. of QM which provides a classical, local explanation of contextualism in H-V theories. His ideas are very unorthodox but by introducing what is essentially a medium he seems to be able to avoid that strange modern marriage of hidden variables and Bohr's wholeness idea.

My course on Carnap's Inductive logic is over and I'm getting ready for a course on philosophy of physics next semestre. I look forward to your comments on the Disturbance stuff.

Best wishes to you and Jenny,

Harvey.

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Centro de Lógica, Epistemologia e História da Ciência
Universidade Estadual de Campinas - UNICAMP C.P. 1170
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Brasil

Campinas, 12th July, 1979.

following. In accordance with the suggestion of Prof. Coffa, a special number will be published on recent developments in the problem of analyticity in the philosophy of Rudolf Carnap. Another special number will be dedicated to ancient geometric analysis and its modern reconstructions. Within the imminent series of regular numbers, there will appear contributions in the field of cognitive psychology, by such authors as Staddon, Putnam, Nelson and Chomsky. A similar series of articles, also distributed over the regular numbers, is planned in both the history of methodology and in the set-theoretical approach to the structure of physical theories. Finally, two special numbers in the areas of the human sciences and problems in quantum mechanics respectively, will contain contributions to the first *Encontro* of December, last year. This is an annual symposium on themes in the history and philosophy of science organised by the *Centro de Lógica*.

Apart from any comments you may have on the above, we would be grateful for any suggestions concerning future publications. These may concern specific texts for any of the mentioned sections of the *Cadernos*, topics for special numbers, suggestions for series of articles. Finally, we would be glad to receive any contributions of your own which you consider appropriate for any of the sections. The decision of the editorial board to publish such contributions would follow standard practice.

We are extremely glad to have your support in this venture.

Sincerely,

Harvey R. Brown

Harvey Robert Brown
Secretary of *Cadernos*

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Aldo Chiorato 11,
Jardim Taquaral,
Campinas, São Paulo
1 May

Dear Michael,

I am at last in a position to send you the summary of our work on the disturbance theory. I do apologise for the length of time it has taken me to get the thing written. When I last wrote to you, I didn't realise how time-consuming my course in the Centre would be: what with the material being largely new to me and the problem of giving it in Portuguese, the task of organising the lectures was a full-time job. However, two weeks ago all the public funcionarios in the State of São Paulo went on strike (the first such activity in fifteen years). ~~and~~ Unicamp being a State University, this includes all the teaching staff in strike. So I was able to use this breathing space for the paper.

Some remarks about the paper. First, I apologise that it is hand-written, but to wait to get facilities for typing it at this point would delay getting it to you considerably. Also, I imagine that you may want to alter it in places or suggest ammendations etc. Secondly, I realise it may be too long for example, the Am J. Phys. I tried to compress the material as much as possible, but it ~~is~~ may be susceptible to further 'cutting'! In this respect I would be grateful for any suggestions you may have; you may also feel that some points have been omitted. There are three points in particular which I would like to draw your attention to; these are points I doubt that we ever discussed in detail. The first is my account of Park's anti-DTI argument in Section II. Do you agree with the line that the DTI is essentially divorced from the Projection Postulate? The second is the difference alluded to on page 9 between the spring-collision model and the coupled spins case. Since the ~~former~~ former has a natural finite interaction period τ (the compression and relaxation of the spring) corresponding to the time of the complete state-preparation procedure of particle 2, it seems to me the fact that the DTI does not hold for $t < \tau$ is not a strong objection to its applicability here. Finally, there is the last ~~page~~ paragraph in Section III concerning the purported connection between the DTI and

yes

yes

Well Checked? the randomisation process in HV theories in QM. Do you find the argument plausible?

You will notice in Section III in the quasi-classical treatment some deviations in notation from the text of my thesis. I use $\underline{L}_1$, $\underline{L}_2$ instead of $\underline{N}$, $\underline{M}$ to keep notation more consistent with the quantal treatment, ~~and interchange the role of the systems (System 1 is now state-preparer)~~. I realise that the terminology is a little more cumbersome in places as a result. Also, I don't develop the rigorous solutions in Case I (Section III) because as I try to point out the impossibility of the DT1 is seen immediately. Anyway, the full solutions in Case II are ~~more manageable~~ simpler and I think more striking. I reconfirmed in calculation the results for Case II.

Also I would like to know what you think of the informal argument against the DT1 presented in the first paragraph of Section IV.

Good

Finally, feel free to make any changes in the English in the paper (this goes without saying).

2nd May.

Regarding the question of length again. I notice that articles in recent editions of the Am J Phys are in general very short; how would you feel about publishing in Foundations of Physics? (I've yet to find a physics library in Brazil that takes it!)

I have also come across an article in Physics Abstracts which may be relevant to section II, and in particular the Bohm-Aharonov argument against the DT1. It is

Y. Yoshihuku 'Can the apparatus disturb the observed system without interacting with it?' Mem. Chubu Inst. Technol. (Japan) 13A 173-5 (1977). *

I can't get hold of the journal here; in fact it seems that the paper may be concerned with some technical problem in Freedman and Clauser [1972]. I mention the paper just in case you feel its worth seeing if there is any in it germane to our position.

* Ref. no. 89361 in Physics Abstracts.

In the same vein, there is a recent article by Janossy on the tunnel effect in QM which may again conceivably be relevent to the last argument of Section II. ~~Unfortunately the reference~~.

L. Janossy Foundations of Phys. 8 119 (1978).

You may however feel that we have said enough in this respect.

I think thats all concerning the paper for the moment.

All goes well in the Centre. In the next semester beginning in August we will have visiting us Joseph Sneed and O. Costa de Beauregard for some weeks. The latter ~~which~~ will teach part of a course I will give on the foundations of physics, as well as give seminars to the physics department. I'm now involved in giving a course to the masters students on interpretations of probability which will lead to a discussion of confirmation theories in the philosophy of science.

I hope all is well with you. Have you written up your ideas on the Sum and Product Rules in complete value assignments in QM? I'd like to know your conclusion.

My best regards to the family. I look forward to your reactions to the paper.

Harvey.

Janossy shows about a non-conservative energy in tunnel effect is due to an extra stored 'wave' energy Q. – based on his hydrodynamical model of QM

Heisenberg p. 23 discusses x-p uncertainty relation
induced by ~~pos~~ diffraction of an electron beam
by a single slit

p. 24 " In this discussion we have avoided the
dual character of light, but have made extensive
use of the two theories of the electron".

cf discussion of Mercado on p. 21/22 above "simultaneous
use is made of deductions from the corpuscular and
wave theories of light" (foot of p. 22.

A Heisenberg in fact uses two sorts of argument in the course of his discussion.

B However in discussing the diffraction of an electron beam by a slit Heisenberg derives the uncertainty relations for the electrons emerging from the slit using quantal aspects of the electrons[2], not of the ~~slit~~ diaphragm containing the slit.

In subsequent discussions ~~the source of the uncertainty relations has often been ascribed to interference of the first mechanism, the second being ignored. In example consider the~~ of the single slit experiment by Bohr[1] known as quantal aspects of the diaphragm with containing the slit were regarded ~~[illegible]~~ as responsible for the momentum uncertainty of the energy beam. So Heisenberg's second set of examples is apparently reducible to the first. We shall argue in what follows that these additional examples offered by Bohr as

1 cf, eg, Bohr [1949] p. 214-15.

2. Heisenberg [1930] pp. 23-24.

essentially misleading as ~~a source~~ an explanation of quantal uncertainty for the electron in the single slit experiment and that Heisenberg's own analysis in terms of quantal effects of the electron is the line that should be followed in discussing the uncertainty relations in the case of the ~~Doppler~~ other experiments such as the Doppler measurement - measurement are.

Indeed Bohr himself in later writings retreated from the disturbance doctrine as an explanation of the uncertainty relations, emphasizing the wholeness of a quantum phenomenon involving the specification of the experimental arrangement in classical terms, as rather than mechanical transmission of uncontrollable disturbance as the source of the characteristic features of the theory. ~~According to Scheibe~~ This shift was offhandedly to be the difficulties of understanding the EPR discussion in terms of a disturbance theory[1]. We shall comment on this in section II below.

---

[1] Scheibe [1973] ~~pp. 20 and ff.~~ p. 20 ff. and p. 26
and Bohr [1935] p 700

A version of the disturbance theory can also be found in the - - - - -

S All that we can infer is that $\langle v(x) \rangle$

L $\langle H(x) \rangle$ so that the ~~electron~~ particle can never be confined entirely in ~~a region~~ the classically inaccessible region.

P ~~For small θ, $|P_1^t|$ is too small by a factor of order θ^2. This might~~ In contrast to the linear collision example ~~indeterminacy~~ continuity uncertainty is not retained once it has been achieved but is ~~constantly~~ lost and regained in a cyclical manner. This difference in behaviour follows from the different properties of the two interaction processes: the spin-orbital linear collision and the periodic precession of the two spins about their vector sum.

Footnote E We may note in passing that the clean indeterminacy wobbling of the two spins in our ~~special~~ example

is due to the fact that the initial uncertainties in $\underline{L}_2$ are chosen in such a way as not to affect $|\underline{L}_2|$ which controls the speed of precession. In general this would not be the case. We have chosen the simplest example that reproduces the main features of the quantum case, time-dependence of $\langle L_{1z} \rangle$ with an initial value of zero and clear indeterminacy surrounding

~~Footnote~~

We are maintaining also that there is no inconsistency in allowing interaction between a quantal and a non-quantal system. However the detailed theory of such an interaction ~~is of course~~ would of course be subject to certain constraints. Taking again the case of the single slit experiment and a non-quantal slit we could not correlate a single position and a range of momentum values ~~variables~~ of the slit simultaneously with a position and momentum corresponding variables of the electron – indeed the momentum variable of the slit might now have to be equated with the expectation value of the momentum for the electron. Thus compensation of momentum would be ~~dy~~ ~~Ad~~ satisfied only on the average as between slit and electron.[1] ~~However if the slit possesses a sharp momentum and the electron being quantal has no sharp momentum~~

Incidentally the source of the indeterminacy relations is not or indeed between systems with different values for $\hbar$ – cf Menzel [1964] p149 who argues that this would lead to inconsistency.

[1] However such questions are matters of empirical investigation, not of a priori argument.

to be sought then in disturbance by interaction with ~~one~~ other quantal systems, but rather in an inherent property of the original system along the lines suggested by Heisenberg's original reasoning about the single slit experiment. ~~It is clear that~~

Instead of redoing ~~the~~ argument in this case ~~case~~ to be extended to the situation in the other gedanken experiments as suggested by Bohr we cannot recommend exactly the reverse amendation.

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Campinas, September, 26, 1979.

Dr. MLG Redhead
Dept. History and Philosophy of Science
Chelsea College, Marnesa Rd.,
London SW-3

Dear Michael:

Many thanks for returning the paper with so much interesting commentary, and for the copy of Yoshihuku's paper. I very much endorse the suggestions for alteration and the additional material, with some very minor exceptions which I will return to in one moment.

I have made reference to the articles by Roychoudhuri and Yoshihuku in the following manner. I am including a footnote to the former immediately after introducing the microscope-experiment of Heisenberg in section I:

"Recently, Roychoudhuri (1978) has remarked in relation to the microscope experiment that later improvements in resolution techniques render the Rayleigh criterion for the finite resolving power of the microscope overly restrictive. It was the Rayleigh criterion that Heisenberg, following a suggestion by Bohr, used is deriving the indeterminacy relations. In such cases of "super-resolution", in principle Heisenberg's general argument may be employed to violate the indeterminacy relations."

In reference to Yoshihuku, I am including the following footnote immediately after the critical discussion of the reconstructed Bohm-Aharanov argument against the DTI in Section III: "Another argument, very similar to that of ~~Bohm and Aharanov~~ correlated systems, which is based on the negative-result-type experiment of Renninger (1960), may be formulated against the DTI. in the celebrated two-slit experiment, if a detector placed at one of the slits fails to register the passage of the particle through that slit, this result is tantamount to the detection of the particle at the remaining open slit. A specially chosen ensemble of such negative results would not yield the interference distribution at the detection screen, and thus the mere presence of the detector at the original slit is said to disturb the system without the detector entering into physical interaction with it (This is essentially the principle underlying the recent argument against disturbance as a physical interaction by Yoshihuku (1978) although the author uses the more complicated example of a double Stern-Gerlach experiment).

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For related discussions, see the account in Jammer (1974) pp. 495-6.) However, as in the Bohm-Aharanov argument, this disturbance does not create indeterminacies in the manner which is of interest to us, and is similarly the physical expression of the Projection Postulate, here identified with the collapse of the wavefunction at the unguarded slit."

Let me know~~s~~ if you feel that these footnotes are not worth including, or should be shortened.

On reflection, I agree that my final comments on the connection between the DTI and the randomisation process in H V theories are vague. I think now that this ad hoc process in H V theories is really the H V - equivalent of the Projection Postulate, and we argue that there is essencially no connection between the Postulate and the DTI, as we define it. However, I still tend to feel, with Bub, that we accept this "disturbance" in measurement in HV - theories because we are used to this kind of language in QM!

My only query has to do with your footnote F. This footnote follows your earlier remark, I take it, that "we can not just equate a sharp quantity with an unsharp one" in a theory of interaction between a quantal and a non-quantal system. Your comments in footnote F make more sense to me if the quantal system is truly quantal, i. e. if the theory of the system is fully quantum mechanical. However, if the interaction is of the quasi-classical variety, where the 'quantal' agent has quantal indeterminacies at a certain time, but nothing more in the way of quantal structure, I cannot see why full-blown conservation laws, such as that of momentum, cannot be applied. Indeed this is exactly what is ~~proposed~~ presupposed in our examples of particle collision and coupled spins. However, even in the former (non-quasi-classical) case, the objection of equating sharp and unsharp values only seems to hold if unsharp means literally fuzzy; but in a statistical interpretation à la Ballentine this isn't the case. Or am I missing your point?

I will get the revised manuscript off to the typist in a few days and send you a copy as soon as it's ready. It wasn't clear from your letter whether you want to see the copy before I submit to Founds/Phys. or not.

I insist on putting your name before mine in the title. The reason is simple: the central ideas are yours.

I hope your conference in Germany went well. I'm sorry I didn't

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have time to make comments on your Sum Rule paper. I've been very busy with my course in QM here in the Centro, and with helping to organise this year's conference on Philosophy and History of Science. In November, we will have Costa de Beauregard with us for a month, who seems to be anxious to discuss the results of recent colloquia in Europe on EPR. Also, I've had Joseph Sneed living for the past two months in my house - he has enjoyed his stay here - found the students exceptionally bright and interested, and found a lot of interest in his ~~wrote~~ work on axiomatisation in physics in the Centro. I haven't attempted to follow carefully his courses - his approach requires rather a lot of technicality, the benefit of which is not easily apparent to me, but he is an extremely interesting man with wide interests.

In answer to your question, I will shortly get round to writing a draft of my ~~stiff~~ study on insolubility proofs. Our fledgling journal - Os Cadernos - wants a short version of the paper in Portuguese soon, so this is an added incentive. On top of this I will write an historical/philosophical introduction to translations of the EPR paper and the Bohr reply for the arquive section of one number of the Cadernos - I'll send you a copy. I have the feeling that Jammer's account of the Bohr-Einstein debate in his (1974) is a little misleading.

My best wishes to Jenny and the boys,

Regards,

Harvey.

P.S. Your equation in Part III, in the QM Treatment,

$$[\underline{\sigma}.\underline{A}\ ,\ \underline{\sigma}] = 2i\,\underline{\sigma}\times\underline{A}$$

holds for any vector operator $\underline{A}$ which commutes with $\underline{\sigma}$. Of course, this is sufficient for us.
I haven't been able to get hold of Bunge's article yet. H.

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Centro de Lógica,
22 November.

Dear Michael,

At long last I am sending you the final typed draft of our paper. I had a series of problems in getting the thing typed and having the diagrams drawn. Here in Brazil, time is of little essence! I apologise for the state of typed text – after having it done, I realised that the referencing system was not compatible with that used in Founds Phys, and so had to alter it by hand.

Once you have read the text, could I ask you to make the required number of copies and then send it off to Founds Physics? Thanks. By the way – the last word in the text – "amendation" – is this the word you had written? I would prefer procedure, but if you are happy with it, I have no objections to amendations.

Last week O. Costa de Beauregard arrived for a stay of one month – very gracious elderly man who has been intimately involved in all the recent discussion in Europe on EPR. He's convinced that the answer lies in relativistic QM, using the Fynman zig-zag in order to show a backwards

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causation in time emanating from the measurement of the spin orientation of one of the two ~~ex~~ separated spin-$\frac{1}{2}$ particles (in Bohm & Aharonov's e.g.), which influences the common source of the particles. Beauregard has long been interested in time symmetry in physics ~~[illegible]~~ ~~[illegible]~~ and regards the backwards causation as a perfectly ~~straightforward~~ outcome of taking seriously the full Lorentz group in rel. QM. He makes rather a good case for his amazing position, as far as I can see.

The other fascinating and unorthodox thread in Beauregard's thinking comes from his growing conviction, ~~[illegible]~~ originally due entirely ~~from~~ to the attempt to come to grips with relativity and QM, in the truth of some aspects of parapsychology and telekinesis. One again, he makes the case embarassingly well! In fact, he is not saying much more at times than what von Neumann, Wigner and Shimony have hinted at, if not said explicitly, in relation to QM. He simply feels as a scientist that for theoretical and pragmatic reasons, these things are worth looking into. In fact, he believes the way to test his hypothesis of backwards causation in the EPR correlation is with the use of a <u>sensitive</u> placed along the path of one of the systems. He knows that such thinking is considered by

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31 October.

Dear Michael,

Thank you for your letter with the comments on the hybrid-type theory. I think now that I've got the sense of the thing. What you can't equate, as you say, are c-numbers and q-numbers, this being quite independent of the question of ~~[illegible]~~ matching 'possessed' values of observables in the quantal part (should they exist) with values of corresponding variables in the non-quantal part.

(The paper will be typed by tomorrow, so that I can send it and copies to you after drawing the figures).

There is one item in the question of the necessity of quantising, say, the radiation field, which still bothers me — something I think we discussed tangentially in London but never thrashed out. It's the old informal argument of Heitler that using classical wavepackets in a scattering experiment to "measure" the position of a quantal particle, one can in principle violate the uncertainty relations. (This argument was recently ressuscitated by Eppley and Hannah Founds Phys 7 (1977), 51). What is troublesome is not that conservation of momentum, say, may only be preserved in the mean in the hybrid theory, but that by manipulating the scattering experiment one can in principle select a post-interaction ensemble of the q-particles for which the indeterminacy relations are violated. ~~T~~ Of course,

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a purely formal; hybrid treatment of the scattering would of necessity be consistent with the indeterminacy relations for the quantal particle, but I fail to see the ~~informal~~ hole in the informal reasoning of Heitler, and Eppley and Hannah.

There is a request I would like to make. We have a young physicist who works in the Centro who has an accumulated sabbatical leave of 6 months from the University starting in January of next year. He wants to go to London during this period, and would like to study with you and Jon at Chelsea. His name is Waldyr A. Rodrigues, Jr. (The stay in England would be financed by sources in Brazil, clearly). He is especially interested in the foundations of General Relativity Theory, and to some extent quantum theory, and he seems to have a wide knowledge of theoretical physics. Would you be interested in ~~and~~ his coming, and would you be able to provide if not a desk at Chelsea for him, then at least permission to use the facilities at the College? I'm not in a position to evaluate his philosophical work – I don't know it well – but he seems in conversation extremely intelligent. Personally, he is quite agreeable. It is said by some in the Physics department (which he recently left for us for reasons I don't know)* to be difficult to work with (which may be a virtue in that department), but in my dealings with him

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* the reasons not being entirely the wish to work in philosophy of physics.

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there have been no problems.

If you are interested in his visit, you would have to write as soon as possible a letter of invitation to him, extending the right to use the College's facilities. (~~You can send it to me~~). I'm enclosing his Curriculum Vitae, which is in Portuguese and of typical Brazilian dimensions, but you'll have no problem understanding it.

Finally, I'm making arrangements to go to England in late December for my brother's wedding. It would be great to see you and the family – will you be in London in the first week of January? (I can't be certain that the inevitable bureaucratic requirements of the trip can be sorted out in time, but I expect so).

As I mentioned you can expect the typed version of our belated paper in a few days – the typing took ages because the typist speaks no English, and errors were the rule.

Fondly,

Harvey.

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THE HURLINGHAM CLUB,
RANELAGH GARDENS,
LONDON, SW6 3PR

Dear Rev Sessions,

I enclose the Nomination Form for Professor von Fraassen as a Category B visitor for 1980-81. As you will note the application is supported by the Universities of Oxford, Cambridge, Sussex.

Yours sincerely

Rev F. Sessions
Rm 12, Senate House.

Dear Prof. Scheffler,

I am unsure to enquire whether you have yet received my paper 'Experimental Tests of the Gen Rel', which I sent to you in August. As I have heard nothing from you I am wondering if the manuscript may have gone astray.

Yours sincerely

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Cadernos de História e Filosofia da Ciência
Centro de Lógica, Epistemologia e História da Ciência
Universidade Estadual de Campinas - UNICAMP C.P. 1170
13100 - Campinas - SP
Brasil

Campinas, 12th July , 1979

Prof. Michael Redhead
Chelsea College
University of London
Manresa Road London SW3 6 LX
London - England

Dear Member of the Council,

Owing to unforseen delays, the first number of the *Cadernos* is to be published in the second semester of this year, and not in the first as was originally planned. In a series of meetings, the editorial board adopted various editorial lines concerning which we would like to have your opinions.

As far as possible, we intend in each regular number to include contributions in the following areas: history of science, philosophy of the natural sciences, philosophy of the human sciences, and ideology of science. Various special numbers will be dedicated to a single topic or author, accompanied by an editorial introduction. Each number, regular or special, will in principle contain the following sections: articles, archive, discussions, reviews, notices. It is proposed in the archive to publish translations in Portuguese of older texts considered important in the mentioned areas, along with an introduction and commentary. For example, in the first number the archive will contain a translation of Osiander's preface to the De Revolutionibus of Copernicus, with an introduction.

In addition to the regular and special numbers, we plan to publish an independent series of supplementary numbers, containing a single work in one of the areas, whose length or other characteristics recommend separate publication. The first such supplement will contain the bibliographical review of the literature on theories of scientific method by Laurens Lauden, originally published in History of Science.

In reply to questions of some members, we remark that it has been provisionally decided that all material in the *Cadernos* will, for the time being, be published in Portuguese.

Some further editorial decisions already adopted are the

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